THE UNIVERSITY OF WISCONSIN
COLLEGE OF AGRICULTURE

Madison 6

DEPARTMENT OF GENETICS

January 6, 1952

Dr. E. L. Tatum
Biology Department
Stanford University
California

Dear Ed:

Or slant* After wishing you and yours a most happy 1952, I want to ask the favor of a lyophil tube* if you have one handy, of strain 679. For the past six months, we have been exerting ourselves at, finally, preserving our cultures but this one managed to get away. Most everything else has been recovered, checked, and stored. We're using tubes with $CaCl_2$ as suggested to us by Hershey: this works very well for small-scale operations (i.e. about 2000 cultures) like ours.

You might be interested in the immediate reasons for our wanting this strain. In general, we had found no indications of specific incompatibilities for recombination of K-12 cultures. However, Esther picked up a 58-161 derivative that would not cross with Y-10 or any of its further mutants. This culture will, however, cross quite well with Y-10-like segregants extracted from heterozygotes, or with $T\text{-}L\text{-}B_1\text{-}S^r$ recombinants from crosses of Y-10 x 58-161 S^r picked up on TLB_1-streptomycin agar. This may mean that Y-10 and Esther's culture have picked up a common self-incompatibility mutation, so we're checking the ancestry of Y-10. Her culture and Y10 S^r both cross with K-12, so the "mutation" must have occurred since.

I'm looking forward with anticipation to seeing you sometime this Spring, according to the Enzyme Seminar schedule, so will spare you a long letter. Ed Adelberg may have mentioned the replica-plating method to you [you can imagine how much more I would have wanted this technique as a graduate student]. Have you perchance tried it with Neurospora? I've been doing a few experiments lately with actinomycetes, and it has been indispensable in picking up auxotrophs there. In S. griseus, by the way, effective heterokaryosis seems to occur quite readily; the evidence for recombination is still equivocal but, perhaps, encouraging. There isn't much more going on that I wouldn't rather talk to you about later. Zinder is writing up his thesis on the Salmonella "transductions", and, more or less, looking for a job. He was East last week and ran into numerous rumours, disseminated by Delbruck, of big doings with K-12 at Caltech, but everyone seems to have heard the details but ourselves.

One last note: Clifton sent about 20 coli cultures (K- etc.). One of these was interfertile. This objective test should put your serendipity rating at about 20. No mating systems outside the K-12 group have been found yet.

Sincerely,

Joshua Lederberg